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THE  
INDIAN ASSOCIATION  
FOR THE  
CULTIVATION OF SCIENCE

METHODS IN SCIENTIFIC RESEARCH :

Address delivered by

Sir E. J. RUSSELL, D.Sc., F.R.S.

*Director, Rothamsted Experimental Station,  
on the occasion of the first award of the Joy Kissen Mookerjee  
Medal to him, on March, 13th, 1937.*

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In recent years throughout all civilised countries there has been a prodigious amount of so-called research stimulated by the University regulations adopted for higher degrees : the M.Sc., the Ph.D., and the D.Sc., all of which require the preparation of a thesis involving original work. Much of this is necessarily in the nature of students' exercises. It is routine work carried out in accordance with certain rules, and original only in the sense that a game of cards is entitled to that description, simply because no one had ever before held the same sets of cards, or if they had, would have played them in quite the same way. Many men who hold research posts continue doing this

kind of work all their lives, and never get beyond the ordinary rules of the game or outside the usual conventions. This we can call routine research.

There is, however, another type of research which not infrequently breaks through all the rules and conventions, strikes out into completely new lines and makes some new discovery, opening up fresh fields never before studied by scientific workers; this we can call the discovery type.

The number of people capable of doing routine work is very large and almost any intelligent person can be trained to this end, but the number who are able to make discoveries and do research of discovery type is very small; these men are born, not produced by training. Even in countries like England, where the choice can range over the whole Empire, it is frequently difficult to find a man of sufficient quality, and this difficulty is intensified very greatly as soon as limitations of race or community come into play.

The distinction between the two types of work is of the same order as that between the true artist and the mere painter. The analogy, however, between science and art is not complete, because the second rate artist, while he may give



pleasure to large numbers of inexpert people, may do but little to advance art ; while the second rate scientist, provided he does his work honestly and to the very best of his ability, may render considerable services to science.

How does the scientist work ? It is remarkable how quiet he is on this subject. No lectures are ever given to science students on scientific methods even after they have started to do research, and most of them begin simply by doing as they are told ; some indeed never get beyond this stage. The classical method of scientific investigation is to begin by making experiments, then to observe the result, finally to draw conclusions. This method is associated with Francis Bacon, the great English philosopher of the 16th century, who lived within a few miles of Rothamsted ; it is called the method of experiment, observation and inference. It seems to us the most obvious one, but this was not always so. The older method, which you find magnificently illustrated in Plato's Republic, was to discuss the subject at length, hoping to arrive at the truth by the method of many arguments. It was some long time before Bacon's method displaced this older one.

A good illustration of the classical method is

afforded by Van Helmont's experiments made at Brussels at the beginning of the 17th century to discover the source of plant food ; I cannot do better than give the description written by his son. "I took an earthen vessel in which I put 200 lb. of soil dried in an oven, then I moistened with rain water and pressed hard into it a shoot of willow weighing 5 lb. After exactly five years the tree that had grown up weighed 169 lb. and about 3 oz. But the vessel had never received anything but rain water or distilled water to moisten the soil when this was necessary, and it remained full of soil, which was still tightly packed, and, lest any dust from outside should get into the soil, it was covered with a sheet of iron coated with tin but perforated with many holes. I did not take the weight of the leaves that fell in the autumn. In the end I dried the soil once more and got the same 200 pounds that I started with, less about two oz. Therefore the 164 lb. of wood, bark and root, arose from the water alone." The experiment was well thought out and the conclusion seems irresistible. Yet we know it was not correct. There was an unknown factor ; the carbon dioxide of the air entered into the result and destroyed what was otherwise a very good case. That is the weak point of the method in

its original form. There may always be an unknown factor coming into play and upsetting what is otherwise a sound deduction. Van Helmont knew nothing about air or carbon dioxide.

This danger is now met by following up the conclusions. A mental picture of the process is constructed ; this is called the hypothesis. If the picture really represents the fact then certain results must follow. The hypothesis can be tested by experiments. If it stands the test, it is valid ; if not, it must be rejected, and another must be substituted. This method was much developed during the 19th century and it stands to-day as the type of the western scientific method.

No hypothesis ever comes out entirely unscathed from the test. It may answer 99 times, yet break down at the 100th test. Then it must be discarded. It is no discredit for any man to have his hypothesis overthrown. If his work has helped others to clarify their position, if it has evoked discussion and so induced other investigators to examine the problem from another point of view, then it has served its purpose. Indeed, it might be argued that a hypothesis which came out absolutely right at the first attempt, would not help science so much as one which led to

much discussion and had to be considerably modified by other workers. It is no discredit, though it may be a tragedy, to see your hypothesis shattered.

T. H. Huxley once gave as the best illustration of a tragedy the wrecking of a beautiful hypothesis by an apparently trivial fact.

Dalton's investigations at the beginning of the 19th century illustrate very well this further development. He was studying the chemical combination between two substances and showed that it proceeded, not in gradually increasing quantities, but in definite proportions ; one part by weight of carbon would combine with 1.3 parts of oxygen or with 2.6 parts but not with any intermediate quantity. In other words, 3 parts of carbon would combine with either 4 or 8 parts of oxygen. This was a definite fact and he called it the Law of Definite Proportions. The next step was to make a picture of the process, and here is where the genius of the man showed itself. He revived an old Greek hypothesis that matter is composed of atoms and that these atoms can combine one with another, but that the combination with anything less than an atom is impossible. The atoms united to form molecules.

Of all the elements known to him the atom of

hydrogen was the lightest. He made up a table of atomic weights based on these combination numbers, taking hydrogen as the unit, and drew a very attractive picture of the structure of matter, which greatly impressed the scientific workers in the early 19th century and laid the foundation of modern chemistry. Dalton's hypothesis of the atomic structure of matter was tested by many deductions and always stood the test. It was elaborated and became a system instead of a mere picture ; it could no longer be called a hypothesis but was called instead a theory. The two words are sometimes used as if they were the same thing, but there is a very important difference between them. A hypothesis is an isolated mental picture or assumption ; a theory is a connected system of ideas.

Another example is furnished by the work of the Italian chemist Avogadro in 1812. He observed that the densities of the various gases were proportional to their molecular weights ; a molecular weight in grammes at normal temperature and pressure occupied 22.4 litres. This was an experimental fact. His mental picture of the process was that equal volumes of all gases contain equal numbers of molecules and this has become known to all students of chemistry as

Avogadro's Hypothesis. It has been extensively elaborated and ultimately developed into the Kinetic Theory of Gases.

Scientific investigation takes two forms ; both begin in the same way : the discovery of facts. In applied science the inventor proceeds to discover how best to use these facts. In pure science the investigator sets up a hypothesis to explain them and hopes to be able to develop this into some theory so founding a new system of ideas.

The power of creating hypotheses can be developed. Tyndall, a distinguished physicist of the middle of the 19th century, wrote an interesting book, which students of science should read, called "The Scientific Use of the Imagination". But the making of hypotheses has its dangers ; it must be distinguished sharply from the process of drawing conclusions from experimental data. In making a hypothesis help from analogy is often very valuable ; in drawing conclusions it may be very dangerous.

A serious difficulty with any hypothesis is that its author is easily satisfied with it and he may convince others even though it is incorrect. It is characteristic of the human intellect to strive after broad simple generalisations and these when

presented are apt to be accepted without much question.

But the acceptance does not last long ; intellectual activity proceeds in cycles. The 19th century was a period of great constructive development. Broad, simple generalisations, such as the indestructibility of matter, the conservation of energy, the law of evolution were evolved and they captured the imagination of scientific workers by their simplicity and their apparent sufficiency.

The 20th century, on the other hand, has been a time of overthrowing of established ideas and customs, sometimes after critical examination that led to deliberate rejection, sometimes not. But it is safe to say that all of the broad generalisations reached in the 19th century after many years of labour have now been thrown over ; broken down in far less time than it took to build them up.

Yet the positive contributions of the 20th century to science have been very great. In two directions in particular remarkable advances have been made : improvements of observations and the detailed studies of errors.

The improvement in scientific observations has been astonishingly great : instruments, methods, the whole technique of scientific work, have

developed at an unprecedented rate. Instruments have changed out of all recognition. Measurements can be taken with an accuracy and rapidity that would astound the old scientific workers if they could come back and see them. Appliances such as those designed by Sir J. C. Bose, Sir Venkata Raman and others enable properties to be measured which till recently were quite unknown. More important still, it is now recognised that even the best of these measurements is faulty, that the best conducted experiment has errors. To err is human, and modern science recognises that its data are all liable to error and that it can never possibly arrive completely at the truth. Browning, one of the poets who appreciated science more than usual, compared truth to the asymptote of a parabola and human effort to the parabola itself which, though it be continued through time to eternity, will never finally reach the asymptote, though it is always trending that way.

One of the great advances in modern science has been the development of methods for estimating the magnitude of the residual error ; the probable extent of the gap between the result obtained and the truth. A beautiful illustration is afforded by the work of the late Lord Rayleigh,



which led to the discovery of a whole group of new elements : Argon, Neon, Xenon and Krypton. He was determining the density of nitrogen, a thing which many physicists had done before, and he prepared his nitrogen in two ways : from the atmosphere by removing oxygen and carbon dioxide ; and by heating ammonium nitrite. The density determinations did not quite agree. In itself this would have caused but little surprise—no two determinations ever did entirely agree. The characteristic of Rayleigh's work was that the probable magnitude of the error could be calculated and he showed that the difference between the two samples of nitrogen was greater than could be attributed to the errors of the experiment. The nitrogen from the atmosphere was definitely heavier than that from ammonium nitrite. There seemed little doubt that the gas obtained from ammonium nitrite was pure nitrogen, but the gas obtained from the atmosphere might easily be impure and mixed with unknown heavier gases.

Being a physicist and not a chemist Rayleigh did not wish to follow up the subject. He handed it over to William Ramsay, who in a masterly investigation done in conjunction with

Maurice Travers (afterwards at Bangalore) showed that heavier gases were actually present in the nitrogen prepared from the air ; he isolated them and studied their properties. They had never before been observed : they are inert and therefore are not to be found in combination with any element on the earth's surface.

These striking discoveries were the direct outcome of Rayleigh's studies on the small errors of his determinations.

The lesson to the young scientist is obvious : never attempt to explain away an inconsistent result. If the observations are not as you expect, do not try to hide them, but work out the experimental error to see if there is a significant difference to account for : if so, try to make a picture of the process and see where it leads you. This discovery of Argon, Neon and the other gaseous elements had lain waiting for years in many laboratories, but no one had made it. It is now realised that in science the study of the errors is as important as the study of the facts.

Not only errors of experiment but also other causes of variation in results can be studied statistically. For the biologist, statistical methods have become imperative. There is no such thing as rigid fixity of type. No rigid quantitative

definition of any species or variety can be given. No two organisms are absolutely alike: it is always necessary to investigate the limits within which the organisms vary. In consequence of this variation of material no two experiments with any living organism, whether plant, animal or micro-organism, can ever give quite the same results. In comparing one treatment with another there always remains the possibility that the difference in results has nothing to do with the treatment but is the consequence of variations in the organisms under experiment. All this applies with special force in agricultural science. Thanks to the work of Fisher and others at Rothamsted this is now generally recognised and modern agricultural experiments are drawn up in consultation with the statistician and arranged in such form that the results can be examined statistically, so as to find what degree of probability attaches to them: whether they can legitimately be attributed to the treatment or are explicable simply as variations in the material under experiment.

The use of statistical methods has revolutionised agricultural science and given a degree of certainty to the results which was never before attainable. I cannot too strongly emphasise the need for statistical studies in any university

where scientific investigation is being carried out on modern lines.

The improvement of technique has added enormously to the resources of science, but it has introduced the danger of making investigation too mechanical. Modern apparatus is very costly and very delightful to work with ; one feels all the pleasure of a child in handling a new toy, in addition to the dignity of being regarded as a profound scientist, because one can manipulate the amazing-looking piece of mechanism. It is easy to go on grinding out figures and results without thinking what, if anything, they mean. It is difficult to vary the method ; there is a lack of flexibility. A good example is afforded by the work of Warington on the nitrifying organisms. He showed that they are active in the soil, yet could never succeed in isolating them. He learnt the best bacteriological technique of his days and applied it with consummate skill and energy to the numerous organisms he picked out from the soil. Yet none of them proved to be the actual nitrifying organisms itself. He worked on for 20 years, diligently and conscientiously, but without success, and at the end of the time had the mortification of seeing a young Russian, Winogradsky, using completely revolutionary

methods, solve the whole problem in a few months. It is essential to keep the methods flexible, and in the early stages of investigation the apparatus should not be too complex. At Rothamsted we use Meccano a great deal for building up our apparatus : it is very easy to make modifications. Every large laboratory should have its glass blower who is competent to make new apparatus.

There has been also great improvement in the interpretation of results. The old method, the use of logic, is still essential ; unfortunately science students are rarely taught logic, and one not infrequently finds them trying to perform some logical absurdity, such as proving a negative.

Modern methods of interpretation are based on statistical examination of the results. Two statistical methods have proved of great value.

The analysis of variance is the basis of tests of significance and is also used for estimating the magnitude of experimental errors in properly designed experiments.

The analysis of covariance is the extension of this method used when more than one variable is involved : it is based on the classical method of regression, which has now taken the place of

the correlation method at one time much used in experimental work.

In applying these methods to survey and other non-experimental data it is necessary to exercise great care in interpreting the results : many kinds of spurious correlations or regressions can be worked out by ingenious-minded people.

I need not elaborate the advantages of these methods, as fortunately in Calcutta you have Professor P. C. Mahalanobis who is an expert on this subject.

A further way in which statistical methods have helped modern science has been in the design of experiments. In at least three directions the design has been improved :—

- (1) to enable the experimenter to obtain the maximum of information from the experiment ;
- (2) to give a valid estimate of error ;
- (3) to minimise or obviate the difficulties associated with the variability of the material.

A fundamental change in the design of the experiments has resulted from recent work. The old method was to put a single question to Nature in the form of an experiment designed to give the answer yes or no or to bring out some simple quantitative relationship. The modern

method is to mix up the questions and put several at the time. It has been used greatly by Dr. R. A. Fisher in designing modern field experiments, and as several factors are brought in, these experiments are called "factorial" so as to distinguish them from the simple type of the old days.\*

The danger is that the experimenter may allow statistical methods to run away with him. A man with a slight knowledge of mathematics finds great satisfaction in handling some simple formula and in making play with an equation that fits rather badly his experimental curves.

A competent statistician will of course discount these tendencies, but the non-mathematician may easily be overcome by them. When at Rothamsted Dr. Fisher first showed how to allow for missing plots in a series of field trials, some of the young people at our Christmas party got up a song celebrating the exploits of a young experimenter all of whose plots were missing, but he consoled himself by the hope that the statistical department would somehow make the allowances and give him his results.

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\* For a description of these factorial experiments and of their statistical treatment see *The Design and Analysis of factorial Experiments*—G. F. Yates., Tech. Com. No. 35, Imperial Bureau of Soil Science. Rothamsted Expt. Station, 1922.

A further change in scientific method is to cast the inquiries into the form of an extensive and systematic investigation. The older work tended to deal with specific questions ; modern work with subjects. The older procedure can be compared to the making up of songs and ballads ; the modern work to the composing of symphonies and concertos. The modern method is of course much more difficult and we suffer from it in that there is a rather large output of poor work masquerading under the name of science, which in the end, is quite properly buried in some scientific journal where one can feel reasonably sure it will never be read. We have proceeded a long way from the day when Priestley, at the end of the 18th century, could compare scientific investigation to the old time hunt, where men would go out to catch whatever they could in any haphazard way, and where anyone with a little skill and some luck could find something. He himself was very successful at this "hunting" business : *e.g.*, he heated oxides to see what would happen ; among them was mercuric oxide and so he lighted on the discovery of oxygen, which led to the foundation by Lavoisier of the modern science of chemistry. Some men have a genius for this kind of experiment, but for most people



it is wasteful and inefficient, like firing a gun out of a window on a dark night in the hope of hitting something.

The modern method consists as already stated in developing a subject and it has become a highly organised pursuit. The Universities participate in two ways : many of the staff spend much of their time on research work—it is always a moot point how their time should be divided between teaching and research—and students who wish to obtain higher degrees can do so only by taking part in the research work.

The various mechanical aids to investigation usually enable a man to obtain his doctor's degree without difficulty ; but this does not mean that he can himself do research ; the real test comes later.

One of the great dangers of this elaborately organised type of investigation is its tendency to keep the workers too busy to think about their work or to follow up any observation that does not fit into the general scheme. The great scientists of the past realised the need for quiet thinking or brooding over their results to find out what they might mean. There is a great temptation to have a big programme and to take on many lines of study. This is a fatal mistake. It stops

a man from thinking, makes him a mere recorder and deprives his work of much of the value it might otherwise have.

Side lines are always a difficult problem : some regard them as a nuisance : others find great temptation to follow them up. In a purely technological institute it may be impossible to do this and the difficulty solves itself ; in a scientific institution the investigator must choose. The great scientific geniuses have always been able to recognise the relative importance of the main line of work and the side line, and they have not hesitated to drop the main line when the side line seemed more promising. A good example is furnished by the work of Professor and Madame Curie who were studying the composition of certain minerals and found that a packet of photographic plates lying in a drawer in the laboratory had become fogged. An ordinary investigator would have seen no connection between the minerals and the plates : they, on the other hand, followed the matter up and discovered radium.

Few scientific workers have either the time or the inclination to stop and think what lies behind their work ; its philosophic background is commonly ignored. Yet this philosophic back-

ground is extremely interesting to a man who will take the trouble to understand it. The old idea was expressed in the law of causation ; the same causes, it was said, always produce the same effects. But like other simple laws of the 19th century this is no longer considered adequate. One difficulty is that the causes do not remain constant and their effects depend on conditions which never again can be made entirely the same. Again the statistician has come to the rescue, and while the 19th century scientist thought of the "laws of nature" as something fixed and immutable, the 20th century scientist seems rather to be moving to the position that there are no fixed laws and one can speak only of the habits of nature.

A completely different scientific method has been adopted in Russia since the revolution. When so much else was being overthrown there was of course no reason why scientific methods should remain exempt, and the writings of Marx and others revealed a system called dialectic materialism which differs in many ways from the older western methods. Instead of proceeding by experiment, observation and inference its method is by thesis, antithesis and synthesis. It is an extremely complex system and the untrained

man early goes wrong in handling it. An account of it is given by J. G. Crowther in his book "Soviet Science".

Finally; may I give a few words of advice to the students before me, many of whom probably wish to develop their powers of scientific research ?

• First and foremost : study carefully some of the great classical papers in science so as to see how the masterminds have done their work. Do not, as so often happens, confine yourself to papers in your own subject : study others also ; here are a few in alphabetical order that every well-read science student should know :

Horace Brown's paper on The Diffusion of Gases in relation to the assimilation of carbon by plants.

Einstein's popular book on Relativity.

Emil Fischer's papers on the Synthesis of Sugars.

Pasteur : On the Diseases of Beer.

Rayleigh : Density of Nitrogen.

Rutherford : Radio-Activity.

Winogradsky : Nitrification.

Before you begin your research think out carefully what you are going to do. Be perfectly unbiassed. Do not set out to prove something

but to test something. As the work proceeds set out your results periodically and think about them seriously. Make an early and careful draft of your paper even before the results are completed so as to ensure that you understand what you have done and can explain it to others, and, more important still, that you see what the results mean. For most scientific students this is a serious difficulty, because so few have had much opportunity of learning any language thoroughly. I do not know how many science students in the Calcutta University could write up their results in literary Bengali, in words that would accurately represent what they had actually done and what they thought the results really meant. Certainly many English science students are quite unable to express themselves in literary English. No better guides to good English can be had than Quiller Couch's "Art of Writing" and Fowler's "Modern English Usage".

Try to build up your work into one decent paper. You can not be sure of writing a classical paper, but at any rate you can try. In particular, avoid the bad habit of fragmenting your work and scattering the material for a good paper into a lot of little ones. Reputations can

never be built up on multiplicity of unimportant papers ; a few good ones are far more effective.

Do not take up too many lines of work : choose one, stick to it and do it thoroughly. If you are not yourself working, but supervising others, you may supervise two or three lines of investigation, but it is not likely that you can do more. A man who scatters his energies over too many subjects cannot possibly acquire full critical knowledge of the details and so is almost sure to fall into some error.

Lastly—and this is particularly important—make sure of your fundamental assumptions before you proceed to build upon them. Do not use a method before you are satisfied that it is really valid ; and above all do not begin to investigate some phenomenon until you know that it really exists. Years of work have been lost through seeking to explain something which afterwards turned out to be a faulty observation.

The path of the scientific investigator is beset with many difficulties and disappointments ; frequently it leads to no financial or social success. But it brings its own rich reward and a man who faithfully devotes himself to the work need never have cause to regret his choice.

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